SCIENCE

VOL. LXV FEBRUARY 4, 1927 No. 1675

CONTENTS

The American Association for the Advancement of Science:
Research as a Method of Education: PROFESSOR A. J. CARLSON 125
The Bermuda Biological Station for Research: PROFESSOR HERBERT W. RAND 128
A Counterfeit Collection of Mexican Plants Falsely Attributed to Brother G. Arsène: DR. PAUL STANDLEY 130
Scientific Events:
The Harvard African Expedition; The New York State Psychiatric Hospital and Institute; The Fortieth Anniversary Celebration of Arthur D. Little, Inc.; Dr. Coolidge and the Edison Medal; Popular Lectures on Science 133
Scientific Notes and News 135
University and Educational Notes 139
Discussion and Correspondence:
The Quantitative Theory of Sex: DR. OSCAR RIDDLE, DR. ALEŠ HRDLIČKA. *Some Notes on the "Elden Pueblo":* PROFESSOR HAROLD S. COLTON. *Presbyopic Vision as an Index of Longevity:* PROFESSOR C. J. ELMORE. *Simplified Spelling:* A. H. BABCOCK 139
Scientific Books:
Russell, Dugan and Stewart's Astronomy: PROFESSOR R. T. CRAWFORD. *Herculano's History of the Inquisition in Portugal:* DR. DAVID STARR JORDAN 142
Scientific Apparatus and Laboratory Methods:
Method for Estimating Glutathione in Insects: DAVID E. FINK 143
Special Articles:
The Oilfish, Ruvettus Pretiosus, at Bermuda: DR. E. W. GUDGER, L. L. MOWBRAY 145
The Northwest Scientific Association 146
Science News x

SCIENCE: A Weekly Journal devoted to the Advancement of Science, edited by J. McKeen Cattell and published every Friday by

THE SCIENCE PRESS

Lancaster, Pa. Garrison, N. Y.

New York City: Grand Central Terminal.

Annual Subscription, $6.00. Single Copies, 15 Cts.

SCIENCE is the official organ of the American Association for the Advancement of Science. Information regarding membership in the Association may be secured from the office of the permanent secretary, in the Smithsonian Institution Building, Washington, D. C.

Entered as second-class matter July 18, 1923, at the Post Office at Lancaster, Pa., under the Act of March 8, 1879.

RESEARCH AS A METHOD OF EDUCATION[1]

THE following comments are based on some thinking as well as on some experimentation in a restricted field, but I claim no originality either in the analysis of the problem or in the remedy proposed. For many years I have practiced the principle that every medical student, at least, should do a bit of research in some one field as a part of his professional training, in order that the scientific method may, as a conditioned reflex, become a part of his daily thinking and behavior. I have urged this as an educational method making for saner men and not with the expectation that any considerable number of the students doing this as a matter of training will develop into professional investigators in the medical sciences. I have practiced the method, despite the fact that a conditioned reflex, however firmly fixed, is soon effaced when not in daily practice, largely on account of the additional fact that some kind of residual effects in the nervous system remain for a long time and render it easier to evoke the processes anew. The discussion is presented here because research as an educational method does not appear to be given universal consideration in the analysis of the educational problems of to-day as affecting primary, collegiate and professional schools.

There is some dissatisfaction with the results of present-day formal education, on the part of the educators, on the part of society, and possibly on the part of those in the process of being "educated." In consequence of this, our educational methods are being subjected to destructive and constructive analysis as never before. Notwithstanding the dissatisfaction, we have extension of formal education to more and more people; the period during which young men and young women are subjected to our formal educational processes is gradually lengthened, we are making increasing financial investments in educational institutions, and despite the criticism we still appear to have an abounding faith that education will somehow save society from many of its errors and follies. Funds—public and private—devoted to formal education in most countries in the world are increasing at a greater rate than the population; and, as an ex-

[1] Address of the retiring Chairman of Section N—Medical Sciences, of the American Association for the Advancement of Science, Philadelphia, December, 1926.

ample of the increase in the time required for formal educational processes in this country we cite the fact that within the last thirty years the course (including college requirements) leading to the degree of doctor of medicine has been lengthened from three to seven, and in some schools, to nine years. The implicit faith in formal education as a social panacea is most commonly seen in the professional educators and in our so-called statesmen. We are adding to the "courses of study" every year, and scrapping very little of the old. We have instituted an "educational test" for the immigrant, though it might be questioned whether a Wassermann test would not be a better index of fitness. But even the educator has moments of doubt and in other sections of society we encounter now and then not only doubt but aggressive challenge.

An "educated" man is supposed to have a certain fund of information plus a certain controlled behavior or disciplined emotions. He is supposed to have acquired a certain degree of critical judgment as a matter of automatic cerebration; a certain method of arriving at conclusions by analysis. He is supposed to weigh evidence, to keep an open mind in regard to the unknown. When we survey the behavior of humans to-day, it is very obvious that critical judgment, except in matters of immediate practical interest to the individual, is largely conspicuous by its absence. The scientific method has not yet become a tool in everyday human behavior. We know more facts than ever before, but, on the whole, we do not seem to be much wiser, more sane and more just than our more ignorant ancestors. This applies to the so-called educated members of society as well as to those who have come in the least contact with the formal educational processes of to-day.

Present-day educational methods are largely devices for imparting information. Owing to the accelerating curve of research in the objective sciences, practical applications of the results of such research to industry, "quantity production" in the arts, etc., the number of facts (and opinions) are annually increasing to such an extent that the attempt to implant all this ancient and modern information in the brains of youth leaves little time for anything but memory cramming. The present-day educational methods appear to be designed to impart the maximum of information in the minimum of time. As a necessary corollary to this method comes the importance of authority. We learn by doing, and effectively, only by doing. But in our formal education the young generation is seldom permitted to learn by doing. This takes too long time, it is too wasteful of material resources, too many mistakes will be made, because, inevitably, inexperienced youth will go through many of the devious paths of error which have been explored and rejected by the adult and so-called educated generation. *Education by dictation* starts in the home. Parents say, "thou shalt," and "thou shalt not"; "this is so," and "that is not so"; and some of it may be true. The fault, nay, the tragedy, is that parents have not the patience, or, in some cases, the ability to take the child through the processes by which this or that particular thing has been proved to be so or not so. In many cases the parents have not the ability to do this because they themselves have received the "so" or the "not so" as an earlier fiat of their own parents, their own priests, their own schoolmasters, their own college professors. Added to this type of education by dictation in the home during the most plastic period of the human brain, the frequently equally dogmatic teaching in religion and morals, the same type of memorizing and dictation in the "three Rs" (and even in the sciences), in the primary schools, and add to that the four or more years of pretty much the same memory drill and *ex cathedra* teaching in the college—what may we expect from our educated youth? Information? Yes! Belief in authority? Yes! Mastery of the scientific method? No! Conscious or subconscious application of scientific method in the behavior of everyday life? Certainly not, except through "extra mural" experience. We can not develop the method of rational behavior, scientific approach and handling of the problems confronting us without at least in some field facing the problems and learning for ourselves by errors and success in the solution. The child raised in the well-to-do city home, with tutors and preceptors, primary schools, public or private, culminating in four more years in the average American college, may at the end of that process not have faced, not have been compelled to solve, a problem equal to even the simplest that confronts the rat in the experimental maze, except for devices to escape authority. His effective education may thus come largely through rebellion against education itself.

Even students in the colleges or professional schools, preparing themselves for the so-called scientific professions (engineers, chemists or physicians) may, and usually do, go through the four years of college, and the four or more years of their special professional schools, without being compelled, without having the privilege as a part of their formal training of meeting and solving problems where the answer is not known. I speak with some knowledge of the situation in the field of medical education. Here (and I presume this is typical in chemistry and in engineering), the rate of increase of facts and theories is so great that if we expect even the ablest of our students to master them all or even a considerable frac-

tion of them, before he is admitted to the practice of his profession, he will have neither time nor energy to practice science.

Science is a recent adventure in education. Philosophy, mathematics, languages laid down the general methods of education before science was admitted to the curriculum. Now, science itself has largely assumed the easier method of the older university disciplines, the method of memory-cramming, lecturing and spoon-feeding, even for those who are on the way to become more or less professional scientists (engineers, chemists, physicians) instead of holding on to the method by means of which science grows, namely, the method of *doing*.

We seem to proceed on the theory that it is necessary for the student to learn (that is, memorize or attempt to memorize) nearly all known facts in our own and adjacent fields of knowledge before we consider him "prepared" to seriously tackle the unknown in any field. The child is capable of learning by doing as a serious educational process years before he even reaches college. The most effective education I managed to assimilate in the primary grades was received in the manual training workshop. It would be amusing if it were not so tragic to listen to the serious debates of our colleagues on the question whether education by dictation might largely be stopped and something at least different be substituted at the beginning of the freshman year, at the end of the freshman year, at the end of the sophomore, or at the end of the entire four years' college course!

My thesis does not imply that the student at any stage should cease to profit by past experience, should cease to learn from books. It does imply that at every stage of education the student should be permitted, if not compelled, as a part of the program, to answer questions, not from books or lectures, but by field investigation or laboratory experimentation, to the end that the scientific method may become as fixed and as natural in his behavior as breathing.

The weaknesses of my thesis are partly known, and more may develop if a universal attempt should be made to put it into practice. In the first place, there would be some loss of time. The student, left partly to himself to learn, would err, as do even the wisest of adults, in unknown territory. If part of the student's time is to be taken up by making errors and correcting them, he will have less time to cram his brain with "ten thousand useful facts," and this will handicap him in the race with his fellows in all tests based on memorizing these "useful facts." Secondly, the life of the teacher would be made more distracting and laborious. It is easy to lecture to a group of a hundred pupils, to quiz such a group on a textbook or to assign "required reading." Turn the same hundred students loose on problems and the same teacher will have an interesting time, indeed! Thirdly, the introduction of even a small modicum of direct field or laboratory work on the part of every pupil will call for enlarged material facilities all along the line, and tax the ingenuity of the teaching force to the utmost in supplying materials. This will make the plan unpopular with the teacher who is inoculated with the perverted union labor ideal of the least effort that the traffic will bear, but I believe it would make better teachers of us all.

The most serious weakness in my argument is the obvious fact that not all people who have been trained in the method of science by actually doing new work show that this training has materially affected their conduct in everyday life or their judgment in practical affairs. We need not cite examples. They are all around us. This seems to challenge my primary assumption that training and practice in research will establish the scientific method as part and parcel of the behavior mechanism. We have, indeed, the phenomena of "double personality" or "water tight compartments" in the brain, in consequence of which a man may be rational in one particular field, but foolish and unfair when he steps outside of this field, and training in scientific research has not yet eradicated this defect in the scientists themselves. Must we, therefore, abandon the idea that such training modifies behavior in the direction of sanity? Without knowing all the factors in the phenomena, and hence admitting that the question is an open one, I nevertheless venture the following suggestion. Foolish or unfair behavior of normal persons who have adequately assimilated the scientific method is not inherent in the physiological mechanism and therefore irreparable. It is due, in the main, to three factors:

(1) The incomplete control of the emotion by training.

(2) The complex factors and numerous unknowns in many of the practical problems of life confronting us and affecting us personally, and hence the difficulty for any of us to react to them, though we must react, on bases of scientific analysis.

(3) The childhood training and adult belief that certain domain of human relations (*e.g.*, religion, government, sex morality, etc.) are sacrosanct and hence outside the field of the scientific method. This factor would decrease in importance under the type of education here advocated. But it seems probable that all the three factors will affect human behavior for a long time to come.

Accordingly, I do not claim that the introduction of research as a part of education at all stages will make all normal men sane all the time. I think it

will help to make more men sane on more matters for a greater part of the time.

The second unknown in my thesis is whether every person subject to our ordinary educational processes, that is, every child above the moron stage, is capable of profiting by this type of education. There are those who argue that the venture would be a social and economic waste; that at most it should be reserved for the picked few; that all we can do for the rank and file is drill in the "three Rs," in the expectation (I assume) that such "drill" will make for more efficient "hewers of wood and carriers of water." That there are individual differences in capacity to learn, in imagination, in persistence, in practical ingenuity seems clear. We admit, in addition, the facts of heredity. But these facts do not lead to the doctrine of superman and a permanently inferior strata of humans. Speaking as physiologist, with some knowledge of men and a little acquaintance with history, I would say that the "superman" has neither bones, flesh nor good red blood. He is a creature either of conscious fiction or pitiful credulity. And the "superboob" belongs either to organic pathology or is a product of an ill-favored environment plus a perverted educational system. Every normal person would be benefited by a modicum of research at every stage in the formal educational program.

We hear so often that "this is the age of science," and the stereotyped dictum is usually followed by the evidence in the form of enumeration of the striking list of modern scientific discoveries and practical inventions. These achievements of the few have added to the conveniences of the many, but has society thereby achieved greater sanity? Look at the so-called civilized world about you! Scientific knowledge has increased a thousand fold, but we are yet looking for the dawn of scientific understanding in society. The very name science is being perverted to serve superstition, fakery and fraud. The results of scientific research may fill the bystander with awe, just as primitive man stood in awe before the eclipse, the earthquake, the lightning, the rainbow and the phosphorescent sea, but awe does not kindle the cool light of reason.

I am not sufficiently myopic to promise that individual research as a part of education at all levels will be a panacea against all the credulity and unreason of normal men. In urging it as a hopeful experimental therapy, I do not put undue emphasis on the hope, because society will interfere or try to interfere with the experiment. There will be interference on the part of teachers who are satisfied with present methods. And in any event, we start with material already processed in *education by dictation* at the hands of parents and priests.

A. J. CARLSON

UNIVERSITY OF CHICAGO

THE BERMUDA BIOLOGICAL STATION FOR RESEARCH, INC.

IN August, 1925, twelve men interested in the Bermuda Biological Station for Research met at Woods Hole to consider the future of the station. The twelve, all but one of whom had worked at the station, were as follows:

C. L. Bristol	E. N. Harvey
E. G. Conklin	Edwin Linton
M. Copeland	E. L. Mark
E. V. Cowdry	J. W. Mavor
J. F. Fulton, Jr.	S. Morgulis
P. S. Galtsoff	H. W. Rand

The unanimous expression of the meeting was to the effect that a rich semi-tropical fauna and flora such as exist in Bermuda offer ideal opportunities for biological research and that Bermuda's advantages as a location for a biological research station are conspicuous. In Bermuda, reached from New York in about forty-eight hours, this semi-tropical life is accessible to biologists of a large portion of the United States and Canada at a minimum expense of time and money. Living conditions on the island afford reasonable convenience and comfort and freedom from exposure to the dangers which accompany living in a less civilized tropical region. The climate is as nearly as possible ideal. Even in mid-summer, biological work may be pursued either in the field or in the laboratory without serious discomfort.

The first organized biological research in Bermuda was carried on under the auspices of the Bermuda Natural History Society. In the establishment of this society, Professor C. L. Bristol, of New York University, took an active part. Then, in 1903, through the joint activities of Professor Bristol and of Professor E. L. Mark, of Harvard University, with the important cooperation of the Natural History Society and the support of the Colonial Government, the activities of the present station were inaugurated.

The station, while never operated on a large scale, may nevertheless fairly be said to be strongly established. It has been in operation for nearly a quarter of a century. In the course of that time about two hundred and fifty persons have carried on investigations at the station. The investigations, mainly zoological, but to some extent botanical, have covered a broad range of subjects including taxonomy, morphology, embryology, cytology, physiology, biological

chemistry, experimental morphology, parasitology and oceanography. For three years (July 1, 1915, to November 1, 1918) a resident naturalist, Dr. W. J. Crozier, was in charge of the station. While his time was largely given to physiological and biochemical studies, yet his numerous papers include many subjects referring to the morphology, embryology, behavior and adaptation of Bermuda animals. At the close of 1922, the published contributions from the station comprised 141 papers arranged in six volumes, and a seventh volume is accumulating.

In view of the facts mentioned above, it seems evident that, if the station could be provided with an adequate staff and equipment, it would become an instrument of international importance for advance in the biological sciences. About a year ago a group of biologists reached this conclusion, which was embodied in a report placed on file with the National Research Council. It is proposed that provision be made for a resident director and that the station be open to investigators throughout the entire year.

The outcome of the Woods Hole meeting of 1925 was a plan for the complete reorganization of the station. It was proposed to follow, in a general way, the scheme of administration which has proved so successful in the case of the Marine Biological Laboratory at Woods Hole. The ultimate control of the station is to be vested in a corporation composed chiefly, but not necessarily entirely, of working biologists, this corporation to be responsible for the management and welfare of the station through the medium of a board of trustees elected from among its own number.

As a step toward this organization, a list was prepared including about one hundred and seventy-five persons, residents of Great Britain, Canada and the United States, who, because of having done biological work at the present station or for other reasons, might be expected to be interested in the development of the station. To all these persons were sent letters requesting cooperation, and asking nomination of five persons to serve as a committee on reorganization. One hundred and thirty-four persons signified their willingness to join the corporation, and the following Committee on Reorganization was elected:

C. L. Bristol
E. G. Conklin (*chairman*)
E. V. Cowdry (*secretary*)
E. L. Mark
H. W. Rand

The Committee on Reorganization met in New York City on November 23, 1925. The primary importance of securing the cooperation and the support of the Bermudians was recognized. Accordingly, with this in view, a circular compiled by Professors Bristol and Mark, together with a letter from the secretary of the committee, was sent to the governor of the colony, to the former members of the Bermuda Natural History Society (the society unfortunately being no longer active) and to several other residents of Bermuda; also an invitation to join the corporation was tendered to a number of Bermudians who have shown a specially active interest in the station. The responses were, in general, most encouraging and fifteen prominent Bermudians accepted membership in the corporation. During the past winter, Dr. A. G. Huntsman, of the Biological Board of Canada, and engaged in fishery problems in Bermuda, and Professor H. H. Whetzel, of Cornell University, acting as adviser to the Bermudian Department of Agriculture, visited Bermuda and actively interested themselves in plans for the reorganization of the station.

At its meeting of November 23, the Committee on Reorganization arranged for the election of a board of twelve trustees, to be divided into four groups, to serve for one, two, three and four years, respectively. It was provided that four of these trustees should be non-residents of the United States. On February 23, 1926, the secretary sent to all members of the corporation (then numbering one hundred and fifty-one) a letter and ballot for election of trustees. As a result of the voting, the following board was elected:

Non-Residents of the United States:

E. J. Allen, Director, and Secretary of the Council, of the Marine Biological Association of the United Kingdom.
J. H. Ashworth, Professor of Zoology, University of Edinburgh.
A. G. Huntsman, Director, Atlantic Experimental Station for Fisheries, Halifax, Canada.
E. A. McCallan, Director of Agriculture, Bermuda.

Residents of the United States:

E. G. Conklin, Professor of Biology, Princeton University.
E. V. Cowdry, Associate Member, Rockefeller Institute for Medical Research.
C. B. Davenport, Director, Station for Experimental Evolution, Carnegie Institution.
B. M. Duggar, Professor of Plant Physiology, Washington University, St. Louis.
R. A. Harper, Professor of Botany, Columbia University.
R. G. Harrison, Professor of Comparative Anatomy, Yale University.
E. L. Mark, Professor Emeritus of Zoology, Harvard University.
H. W. Rand, Associate Professor of Zoology, Harvard University.

A meeting of the board of trustees was held at the Rockefeller Institute in New York City on April 29,

1926. Dr. B. M. Duggar was elected chairman and Dr. H. W. Rand secretary-treasurer of the board. Papers of incorporation under the laws of the state of New York were approved, and a certificate of incorporation was filed by E. V. Cowdry, B. M. Duggar, R. G. Harrison, E. L. Mark and H. W. Rand. Subsequently, to satisfy the legal requirement that at least one of the signers should be a resident of the state of New York, the name of Mr. Lawrason Riggs, Jr., was added to the papers. Committees were appointed to draft the by-laws of the corporation and to draw up detailed plans setting forth the aims and proposed equipment of the station.

Pending the completion of the legal processes of incorporation, further steps were taken to secure the cooperation of the Bermudian government and residents, and also to elicit the approval and support of the Royal Society, the Biological Board of Canada and the National Research Council.

These efforts to secure cooperation are meeting with most encouraging responses. Under date of July 5, 1926, Dr. E. A. McCallan, director of the department of agriculture of Bermuda, writes that he is interviewing representatives of the Bermuda government with a view to ascertaining the government's attitude toward the station, and intimating that it is highly probable that the government will be willing to make some provision for the needs of the station, possibly in connection with the Bermuda Aquarium, now in process of construction. Dr. McCallan also expresses the hope that close relations may exist between the station and the department of agriculture, and he offers the facilities of the laboratory of the department of agriculture for the use of botanists who may be working under the auspices of the station.

The following is quoted from a letter, bearing the date of July 12, 1926, received from the secretary of the Royal Society:

> At a meeting on the 8th July information was laid before the Council of the Royal Society of London by Professor J. H. Ashworth and Dr. E. J. Allen concerning the steps which are being taken to reorganize the Bermuda Biological Station for Research and to develop it as an international laboratory.
>
> I am directed to express to you the Royal Society's interest in the statement which was placed before them and the satisfaction with which they have learned of the steps which are being taken thus to widen the scope and value of a station which offers such exceptional advantages of position and which, though situated in British territory, had hitherto owed its existence and development to American enterprise.
>
> The Council of the Royal Society hope that they may be kept informed concerning the progress of the scheme.

This letter was followed by one from Professor Ashworth, explaining that the Royal Society's funds are almost entirely restricted for special uses and that the contribution proposed by the society should be regarded not as a measure of the society's approval but as a "Token grant," expressing the society's "interest in and sympathy with the proposals of the Trustees" of the Bermuda Biological Station.

Articles of incorporation were approved by the Supreme Court of the state of New York on June 28, 1926. The certificate of incorporation provides that:

> The principal objects for which the corporation is formed are to establish and maintain a Laboratory or Station for scientific study in Biology; the acceptance and holding of funds, whether from bequest, devise, gift or otherwise, and the application of such funds and the income therefrom to the purposes of this corporation.
>
> The name of the corporation shall be *The Bermuda Biological Station for Research, Inc.*
>
> The territory in which the operations of the corporation shall be principally conducted is in the State of New York and in the Island of Bermuda.
>
> The first annual meeting of the corporation shall be held at the City of New York and State of New York, on the 27th day of December, 1926.

The certificate of incorporation was filed in the office of the secretary of state of New York on June 30, 1926.

HERBERT W. RAND,
Secretary of the Corporation

A COUNTERFEIT COLLECTION OF MEXICAN PLANTS FALSELY ATTRIBUTED TO BROTHER G. ARSENE

A FEW years ago the U. S. National Museum was fortunate in receiving a very large collection of plants made in Mexico by Brother G. Arsène, who was engaged for several years in teaching in the schools of the Christian Brothers in Puebla and Morelia. Brother Arsène is a most enthusiastic botanist, and was enabled to devote much of his time to collecting, with the result that his collections from the vicinities of these two cities amounted to some ten thousand numbers of flowering plants, besides large quantities of mosses, hepatics and lichens. His collections of cryptogams are doubtless the largest ever made in Mexico, and no person has ever made local collections in Mexico at all comparable in size with those which he obtained about Puebla and Morelia.

In his work of collecting Brother Arsène was assisted by other members of the same order, especially

Brother Nicolás, who collected at Puebla; Brother Adole, at Saltillo; Brother Abbon, at Monterrey; and Brother Agniel, at Querétaro; while other brothers made small collections in different regions, particularly Veracruz, the Valley of Mexico and Tlaxcala.

During one of the political disturbances in Mexico the foreign clergy were expelled and compelled to leave the country with only a moment's notice. Brother Arsène's collections had to be left behind, but later, with the cooperation of the departments of foreign affairs of the Mexican and American governments, the collection was forwarded to the U. S. National Museum, to which it had been presented by the collector. Unfortunately, a part of the collection had disappeared through some unknown agency before shipment. The unmounted plants having been arranged by families in bundles, some groups are thus not represented at all in the series acquired by the National Museum.

A full set of these plants was mounted and distributed into the National Herbarium. The duplicates, comprising several thousand specimens, were sold by the writer upon Brother Arsène's account, as a small recompense for the vast work of collection and for the fine series received by the National Herbarium. All or nearly all these duplicate specimens were distributed to herbaria of the United States, and they may be recognized easily by their labels, printed in Washington and uniform with those used regularly in this herbarium (1⅞ by 4 inches). Brother Arsène's original labels, which exist in this herbarium as well as in others of both the United States and Europe, are easily known by the fact that they were reproduced by multigraph, in script.

If the matter ended here, it would be quite simple, but unfortunately some remarkable complications have been introduced and require detailed explanation.

When Brother Arsène went to Mexico, in 1906, he was asked by those in direction in his province of the order to collect as many specimens of plants as possible, and to send them to France, where they might be sold, the funds thus obtained to be devoted to the support of superannuated members of the order who had been deprived of their properties in France. Brother Arsène states that during the years he remained in Mexico he sent to France from five to ten times as many specimens as were received by the National Museum, certainly an enormous amount of material. The plants were received in France by Brother Héribaud, who disposed of them in various ways. A set was sent to the Jardin Botanique de Montpellier, for study by Daveau; several sets were sold to Prince Roland Bonaparte; and most of the remainder were consigned to one or more continental dealers in herbarium material.

The plants distributed by Brother Héribaud were properly labeled, and so far as is known none of the specimens actually collected by Brother Arsène and his associates was ever distributed with an incorrect label.

Mexican plants are in much demand by herbaria, and apparently the supply was not equal to the demand, consequently at least one of the dealers devised a unique scheme for satisfying those who wished Mexican plants. I am informed that one lot of plants purchased by the Gray Herbarium and purporting to have been collected in Mexico by one of the Christian Brothers (Brother Adole) consisted in part of species known otherwise only from Brazil!

The first and only experience that I have had personally with fictitiously labeled plants of this collection was in preparing for mounting specimens of the Buchtien Herbarium, which was acquired several years ago by the National Museum. This herbarium contained several thousand specimens purporting to have been collected in Mexico by Brother Arsène and other members of the Christian Brothers, which had been received by Buchtien in exchange for Bolivian plants. To one acquainted with the flora of Mexico, examination of this "Mexican" collection was dumbfounding.

Many of the plants labeled as coming from Mexico were species of well-known range, of which it could be stated with all confidence that they did not occur in Mexico. Some were from the eastern United States, some from California, others from the West Indies, and some even of Old World origin.

Most of the specimens bearing the characteristic labels that accompanied such material were discarded and destroyed. In looking over several thousand of these plants, it was possible gradually to form some idea of their true nature and source. The results were highly interesting, but the explanation is so complicated that it may be difficult to state it lucidly.

In his desire to have for sale material which he could append to Brother Arsène's genuine collections, some dealer evidently had made use of miscellaneous collections remaining upon his hands, the demand for which had been exhausted. Some of these were from Mexico, others from the United States, and still others from Asia and Australia. All such specimens were now supplied with characteristic and uniform labels, 5¼ by 3¼ inches, bearing in heavy type the heading "Plantae Mexicanae"; the word Mexico, followed by "Puebla:" and "Morelia:"; and "Coll: Nicolas, Arsène."

The dealer who created these false specimens possessed no high degree of ingenuity, else it would not be so easy to expose his system. In some instances

the Buchtien Herbarium contained two specimens of a given rare species lying in a single cover, one being improperly labeled as collected in Mexico by Arsène, the other a correctly labeled specimen of the collection from which the "Arsène" specimen had been fabricated. Any one experienced in the preparation and handling of herbarium specimens will understand how easy it was to recognize the fact that the apparently unrelated specimens had a common origin, since they were alike in every detail of size, shape, discoloration, etc., and in those intangible but quickly perceptible characters which mark all the specimens of a series of one species that have been collected and dried by one collector upon a certain date.

For instance, in one cover was a specimen of *Zinowiewia integerrima* Turcz., *Pringle* 8438, from Sierra de Tepoxtlan, 7,500 feet, September 11, 1900; and along with it a specimen obviously of the same collection, but labeled as collected by Arsène with the data "S. María. 2200. X. 1909." In another cover, *Pringle* 55, *Serjania atrolineata* Wright, from Cuba, collected in 1903, had been divided, and part of it labeled as collected by Adole at Jalapa, Veracruz, January, 1910. Such illustrations could be continued indefinitely.

Part of a specimen of *Arctostaphylos canescens* Eastwood, collected on Mount Tamalpais, California, had been ticketed as collected by Arsène at Rincón. Among the United States collections from which these supposed Mexican collections were segregated are C. F. Baker's "Plants of the Pacific Coast," and his plants of Cuba; plants of the Southeastern States distributed by the Biltmore Herbarium; and various collections distributed by G. L. Fisher. There are many specimens of whose source I have no information, but they include such well-marked species as *Camptosorus rhizophyllus, Woodwardia areolata, Euphorbia ipecacuanhae,* and similar plants that are confined to the eastern United States and do not approach the Mexican border; West Indian plants found by no other collector in Mexico; and California species that reach Mexico only in Lower California, if at all. The only thing retained from the true data is the name of the plant—locality, name of collector, altitude and date of collection all have been changed.

The Old World species attributed to Mexico are rather numerous, but I do not know from what collections they were taken. In the case of these species the distributor had at least the grace not to label them as native Mexican plants, but their origin is indicated usually as in "parcs." Imagination did not fail him, either, for numerous specimens of *Juniperus* and *Cupressus,* of presumably funereal aspect, are indicated as having come from cemeteries! If the clerk who prepared the labels had a sense of humor, he must have smiled when he wrote this. Most of these Old World plants belong to species not known to be in cultivation in Mexico, and the writer has no doubt that the specimens were made in some region thousands of miles away.

The distributor of these plants was not content with ascribing specimens wrongly to Brother Arsène, but his ingenuity was equal to the creation of a new and fictitious collector, Herrera. This is a common Spanish family name, but I have no hesitation in asserting that this particular Herrera never existed. The name selected is not above criticism; Munchausen would have been a better choice.

"Herrera's" collections were manufactured from those of Pringle. In many instances the type collections of Pringle's new species were thus divided. Here, too, only the name of the species was invariably retained. The date of collection is sometimes earlier and sometimes later than Pringle's. The locality is usually the same, but often the altitude (given in feet on Pringle's labels and in meters on those of "Herrera") has been altered.

There are probably other complications that have escaped me, but those mentioned are sufficient to indicate their general nature. The facts of the case are such that no one understanding them can doubt that the labels were falsified with the intent to deceive. I know of no other instance in which similar deceit has been practiced in the distribution of herbarium specimens, and fortunately so, since such deception can result only in chaos. Some of these wrongly labeled collections have reached the United States. Here the result is likely to be bad enough, although in most cases an American botanist will at least question the new records of distribution introduced. In Europe, where knowledge of American geography is naturally less intimate, the results are likely to be extremely unfortunate. Certainly students of "discontinuous distribution" will find much to interest them in the study of these collections. The thousands of specimens included in the Buchtien Herbarium comprise only a small portion of the collection distributed with these worse than misleading labels, for thousands of others were distributed to the larger herbaria of Europe and America.

It is with the hope of warning European botanists as to errors lurking in the labels of the (spurious) Arsène collections and to prevent erroneous records of distribution, which, if printed, will persist for many years, that this article has been prepared. For the benefit of European workers, the following summary of the matter may prove helpful:

Extreme care is necessary in the citation of specimens of plants supposed to have been collected in Mexico by Brothers Arsène, Nicolás, Adole and

Abbon (and perhaps others). If the labels are multigraphed and in script, there is no reason to doubt their authenticity. Labels (1⅞ by 4 inches) issued at the U. S. National Herbarium and headed "Plants of Mexico," with printed locality, also are authentic. Beware of large (5¼ by 3¼ inches) labels, surrounded by a black frame, with the heading "Plantae Mexicanae," and bearing two names of collectors, Arsène and Nicolás, one or both of which are deleted with pen and ink. Plants with such labels are almost certainly false. Either they were not collected in Mexico, or else they were collected by Pringle at another date and locality than that specified. It is best to destroy all plants bearing such labels. All labels of this type bearing the name "Herrera" as collector are fictitious and should be disregarded.

In closing, I can not state in too strong terms that no blame for this condition of affairs attaches to Brother Arsène; rather, he has been made the victim of an unfortunate conspiracy, if such it may be termed. No more conscientious or industrious collector has ever worked in Mexico, and he has contributed in a very large measure to our knowledge of the Mexican flora.

PAUL C. STANDLEY

U. S. NATIONAL MUSEUM

SCIENTIFIC EVENTS

THE HARVARD AFRICAN EXPEDITION

THE Harvard African Expedition began its work in Liberia early in July and left there on November 21. It arrived in Matadi, at the mouth of the Congo, on December 3.

The purposes of the expedition were to make a biological and medical survey of Liberia and to make biological and medical collections there and in the Congo. In the field of medicine the party has obtained valuable data and pathological material which will require prolonged study in the home laboratories. The zoological and botanical collections include biting and parasitic insects, birds, mammals, reptiles, amphibians, molluscs, flowering plants and fungi. The amphibians and the snakes among the reptiles are particularly well represented. The collections of woody plants and orchids are likewise very comprehensive.

Much of the material has been obtained from parts of the interior in which no scientific collecting has been done before and where no medical studies have hitherto been made. In the course of their work, members of the expedition traversed the country in two directions, traveling on foot more than 500 miles and reaching the eastern and southeastern frontiers. Although some of the party were attacked by fever, all are now in their usual good health.

Dr. Glover M. Allen, having completed the zoological part of the work which was planned for Liberia, has returned to Cambridge to take up his duties at the university. The remaining personnel of the expedition is as follows: Dr. Richard P. Strong, Dr. George C. Shattuck, Dr. Max Theiler and Dr. Joseph Bequaert, of the department of tropical medicine; Dr. David Linder, botanist; Mr. Loring Whitman, photographer, and Mr. Harold Coolidge, assistant zoologist.

The expedition will proceed up the Congo and is expected to reach Mombassa, on the eastern coast of the continent, in April or May.

THE NEW YORK STATE PSYCHIATRIC HOSPITAL AND INSTITUTE

CONTRACTS for the construction of the new State Psychiatric Hospital and Institute, to be built in New York on a site provided by Columbia University at 168th Street and Riverside Drive, were awarded on December 30 at the final meeting of the State Hospital Commission, which has now been succeeded by the Department of Mental Hygiene.

The institution will be a center for scientific research into causes and prevention of mental disorders and as a teaching center for the training of mental specialists. In it the research work of fourteen civil State hospitals will be coordinated.

The new building will be of eleven stories. The hospital will provide beds for 210 patients of both sexes. An entire floor will be given over to the children's department, with school rooms, work shops and play rooms.

All varieties of adult mental diseases will be studied. There will be the latest diagnostic and treatment facilities, including hydrotherapy, electrotherapy, occupational therapy, light therapy, physiotherapy, gymnastic psychotherapy and special medical and surgical procedures. Most of two floors will be devoted to the out-patient department.

The tower, rising nine stories above the main structure of eleven floors, will house the library, museum record rooms, doctors' offices, staff conference class rooms and various research laboratories designed and equipped for special studies in neuroanatomy, neurophysiology, neuropathology, clinical pathology, chemistry, bacteriology, serology, endocrinology and experimental psychology.

Being close to the Columbia-Presbyterian Medical Center and medical college, the Institute will provide for psychiatric instruction in connection with virtually all its departments. Its teaching facilities will

be open to the Columbia University Medical School and others, and for the post-graduate instruction of physicians.

THE FORTIETH ANNIVERSARY CELEBRATION OF ARTHUR D. LITTLE, INC.

ARTHUR D. LITTLE, INC., celebrated its fortieth anniversary at a banquet in its laboratories on December 30, which was attended by one hundred or more present and former members of its staff.

Dr. James F. Norris, retiring president of the American Chemical Society, presented the congratulations of the chemical profession, and greetings were read from former associates, many of whom are now prominent in chemical and engineering fields.

Dr. A. D. Little, president of Arthur D. Little, Inc., briefly reviewed the history of his organization, which began business on October 1, 1886, as a firm under the style of Griffin & Little, with office and laboratory on the top floor at 103 Milk Street, Boston. The business which first came to the firm was chiefly analytical, though for a number of years special emphasis was also given to consulting work in the pulp and paper industry in which Dr. Little had previously been active, his initial job having been that of superintendent of the first sulphite pulp mill in the United States.

Mr. Roger B. Griffin, Dr. Little's partner and father of Mr. Roger C. Griffin, chief chemist of the present organization, died in 1893 as the result of an explosion in the laboratory. Six years later the laboratory was moved to somewhat larger quarters at 7 Exchange Place, and the scope of the business was extended.

Shortly after, in 1900, the firm of Little & Walker was organized. The partnership was dissolved five years later, when Dr. William H. Walker assumed the professorship of chemical engineering at the Massachusetts Institute of Technology.

In 1902 another move was made—this time to 93 Broad Street. Here the firm occupied at first half and later the entire sixth floor, but soon the fifth floor also was taken over, and then the fourth, as new departments were established and an organization developed.

In 1909 the concern was incorporated as Arthur D. Little, Inc. There were by this time many specialists on the staff, and departments were maintained for analyses and tests, research, fuel engineering, lubrication, forest products, biology, textiles and chemical engineering.

DR. COOLIDGE AND THE EDISON MEDAL

THE Edison Medal for 1926, which was awarded in December to Dr. William D. Coolidge, assistant director, research laboratory, General Electric Company, "for the origination of ductile tungsten and the fundamental improvement of the X-ray tube," has been declined by Dr. Coolidge for the reason given in the following letter:

SCHENECTADY, JAN. 17, 1927

MR. GANO DUNN, chairman Edison Medal Committee, American Institute of Electrical Engineers, New York City.

My Dear Mr. Dunn:

Judge Morris has just handed down a decision to the effect that my ductile tungsten patent is invalid. This decision, coming from a man of Judge Morris's standing, proves to me that the best of men could question my right to the Edison medal which your committee has been good enough to award to me.

My appreciation of that great pioneer Mr. Edison, in whose honor the medal was established, and my admiration for its former recipients are such that I would not, for the world, do anything that could in any way detract from the luster of that medal, which should stand for generations to come as one of the most coveted prizes for meritorious work in the electrical field.

In the light of the above facts, I can not accept the medal. Allow me to take this opportunity to thank you and the other members of the committee and to express my deep appreciation of the great honor which you did me. Very sincerely yours,

W. D. COOLIDGE

The *Electrical World*, from which we take the above, reports further that at a specially called meeting of the Edison medal committee, held January 21, it was resolved, ". . . with profound regret, to acquiesce in the decision of Dr. Coolidge, which nullifies the award." There will, therefore, be no award of the Edison medal for 1926.

The case referred to by Dr. Coolidge was that of the General Electric Company *vs.* the DeForest Radio Company and the Robelin Piano Company, a suit charging contributory infringement in the manufacture and sale of radio tubes having ductile tungsten filaments, and the court held that the discovery of the cold ductility of the metal was not an invention and that therefore the patent was void. An unusual feature of the judgment was that by it Judge Morris reversed a former finding of his own, made when sitting in New Jersey, which upheld the patent. If his later decision stands, the effect it will have on lamp manufacture has become a subject of considerable speculation.

One of the contentions of the defendants in the suit was that Dr. Colin G. Fink, head of the department of electrochemistry at Columbia University, New York, and a former associate of Dr. Coolidge's in the General Electric laboratories, was the real originator of the process in dispute. Dr. Fink himself made this

claim and had protested to the Edison medal committee of the institute against its award to Dr. Coolidge. In view of the finding that the patent was invalid, the court did not pass on the question of priority of discovery.

POPULAR LECTURES ON SCIENCE

ARRANGEMENTS have been completed for a course of seven lectures on "How the Scientist works" to be given at Manhattan Trade School, New York City, under the auspices of the People's Institute. The lectures will be presented on successive Wednesday evenings beginning February 9. The course was planned by the American Association for Medical Progress, and will be given in accordance with the following schedule:

February 9—*Chemistry, old and new,* Dr. Harrison E. Howe, editor of *Industrial and Engineering Chemistry.*

February 16—*Our knowledge of living matter,* Dr. Robert Chambers, professor of microscopic anatomy, Cornell University Medical College.

February 23—*The life of plants,* Dr. C. Stuart Gager, director of the Brooklyn Botanic Garden.

March 2—*The adjustments of the human body,* Dr. Lawrence J. Henderson, professor of physiology in Harvard University.

March 9—*The chemistry of the human body,* Dr. Carl P. Sherwin, professor of physiological chemistry, Fordham University.

March 16—*The nervous system,* Dr. Louis Casamajor, professor of neurology in Columbia University.

March 23—*How the investigator's mind works,* Dr. William E. Ritter, president of the board of trustees of Science Service.

A similar course entitled "How Science works" has been arranged for Los Angeles, through the cooperation of the Southern Branch of the University of California and the university extension division. These lectures are all to be given by members of the southern branch as follows:

January 10—*The beginnings of science,* Dr. William Conger Morgan, head of the department of chemistry.

January 17—*Revelations of the telescope* (illustrated), Dr. Frederick C. Leonard, department of astronomy.

January 24—*The autobiography of the earth,* Dr. William John Miller, head of the department of geology.

January 31—*The creation of man,* Dr. George M. McBride, head of the department of geography.

February 14—*The modern Aladdin,* Dr. Hiram W. Edwards, department of physics.

February 21—*The chemist in action,* Dr. G. Ross Robertson, department of chemistry.

February 28—*Experimenting with nature,* Dr. Loye Holmes Miller, head of the department of biology.

March 14—*Pathfinding human nutrition,* Dr. Helen Bishop Thompson, head of the department of home economics.

March 21—*What psychology is and is not,* Dr. Shepherd Ivory Franz, head of the department of psychology.

SCIENTIFIC NOTES AND NEWS

DR. ELIHU THOMSON, consulting engineer and director of the laboratories of the General Electric Company at Lynn, Mass., has been awarded the Faraday medal for 1927 by the British Institution of Electrical Engineers. The medal is awarded for notable scientific or industrial achievements in electrical engineering, or for conspicuous services rendered to the advancement of electrical science.

THE King of Italy, through the Italian Ambassador, has conferred the Order of Officer of the Crown of Italy upon Charles L. Parsons, Atherton Seidell and Harrison E. Howe, in recognition of "their friendliness toward the Italian people and their activities in promoting international good-will through the medium of chemistry."

IN the new year honors list of the King of England are included the following scientific men and others connected with scientific work: *Privy Councillor:* The Honorable W. G. A. Ormsby-Gore, under-secretary of state for the colonies and president of section E (geography) of the British Association at the Oxford meeting in 1926. *Knights:* Dr. Henry Head, who has made distinguished contributions to our knowledge of the nervous system; Mr. A. E. Kitson, director of the Geological Survey, Gold Coast Colony; Dr. D. Milne Watson, governor of the Gas Light and Coke Company, London. *K.C.B.* (*civil division*): Dr. G. Macdonald, secretary to the Scottish Education Department. *C.B.* (*civil division*): Mr. H. T. Tizard, principal assistant secretary, Department of Scientific and Industrial Research. *C.I.E.:* Lieutenant-Colonel J. W. Cornwall, lately director, Southern India Pasteur Institute, Coonoor, India; Mr. D. Anstead, director of agriculture, Madras; Mr. D. Milne, director of agriculture, Punjab. *K.C.M.G.:* Professor W. Mitchell, vice-chancellor of the University of Adelaide, in recognition of his services to the Commonwealth of Australia.

CAPTAIN ROALD AMUNDSEN and Lincoln Ellsworth, leaders of the expedition which traveled in the dirigible *Norge* across the North Pole from Spitzbergen to Alaska last spring, have received medals commemorating their feat from the American Scenic and Historic Preservation Society. The presentation took place before a lecture by the two explorers at the American Museum of Natural History on January 21.

DR. W. W. KEEN, professor emeritus of surgery in the Jefferson Medical College, celebrated his ninetieth birthday on January 19.

THE University of Chicago has named the clinic of internal medicine at its new medical school on the Midway in honor of Dr. Frank Billings, professor emeritus of medicine at the university and for many years professor of medicine and dean of the faculty at Rush Medical College.

DR. B. BAILLAUD, director of the Paris Observatory, has resigned.

PROFESSOR A. O. RANKIN, of the Imperial College of Science, London, has been appointed honorary secretary of the British Institute of Physics, in succession to Professor A. W. Porter, who has resigned.

THE following officers were elected at the January meeting of the Washington chapter of the American Institute of Chemists: *Honorary president,* Charles E. Munroe; *president,* Paul H. Brattain; *vice-president,* James F. Couch; *secretary,* J. N. Taylor; *treasurer,* H. L. Lourie.

AT the seventh annual meeting of the Mineralogical Society of America, held at the University of Wisconsin, Madison, Wis., on December 27–29, in conjunction with the Geological Society of America, the following officers were elected: *President,* Austin F. Rogers, Stanford University; *vice-president,* George L. English, New York; *secretary,* Frank R. Van Horn, Case School of Applied Science; *treasurer,* Alexander H. Phillips, Princeton University; *editor,* Walter F. Hunt, University of Michigan; *councilor,* Alexander N. Winchell, University of Wisconsin.

JOHN F. STEVENS, railroad engineer, has been elected president of the American Society of Civil Engineers.

DR. SAMUEL G. BARTON, assistant professor of astronomy at the University of Pennsylvania and acting director of the Flower Observatory, has been elected president of the Camden Astronomical Society, to succeed Dr. John A. Miller, director of the Sproul Observatory of Swarthmore College. The society includes in its membership professional and amateur astronomers in the Philadelphia region.

BRIGADIER-GENERAL CHARLES H. MITCHELL, dean of the faculty of applied science and engineering of the University of Toronto, has been elected president of the board of trade of the city of Toronto.

DR. EDMUND W. SINNOTT, head of the department of botany and genetics at the Storrs Agricultural College, has been elected editor-in-chief of the *American Journal of Botany.* Dr. Sinnott succeeds Professor C. E. Allen, head of the botany department at the University of Wisconsin.

DR. GERALD WENDT, dean of the school of chemistry and physics at the Pennsylvania State College, State College, Pennsylvania, has been appointed by the executive committee of the American Chemical Society as editor of *Chemical Reviews,* to succeed Professor W. A. Noyes, of the University of Illinois, the first editor, who has resigned.

CHARLES M. ARTHUR, of St. Paul, Minn., has been appointed to the editorial staff of the division of publications, Office of Information, of the Department of Agriculture, with title of editor of scientific publications.

PRESIDENT COOLIDGE has sent to the Senate the nomination of Dr. Albert T. Morrison to be assistant surgeon general of the Public Health Service.

DR. W. H. TISDALE, pathologist in charge of cereal smut investigations, U. S. Bureau of Plant Industry, has resigned to go with E. I. du Pont de Nemours & Co., Wilmington, Del.

DR. P. C. MANGELSDORF, assistant geneticist at the Connecticut Agricultural Experiment Station, resigned January 1 to accept the position of agronomist in charge of corn and small grain investigations at the Texas Agricultural Experiment Station.

DR. J. R. HAAG, of the department of agricultural chemistry of the Pennsylvania State College, has been appointed research worker in animal nutrition at the Oregon Agricultural College experiment station at Corvallis.

ARTHUR M. PIPER has been appointed assistant geologist in the U. S. Geological Survey and has been assigned to the water resources branch. S. Spencer Nye, junior geologist in the survey, has been transferred from the geologic branch to the water resources branch.

THE Field Museum of Natural History has announced that Commander Donald B. MacMillan will lead an expedition into the subarctic next summer and establish a base from which arctic exploration and study will be carried on by the museum for a period of five years.

STANLEY F. MORSE, consulting agricultural engineer, sailed for South America on January 15 to investigate soil and sugar cane production problems in British Guiana for an English sugar company. He will also visit the Imperial College of Tropical Agriculture in Trinidad.

DR. E. HORNE CRAIGIE, assistant professor of comparative anatomy and neurology at the University

of Toronto, has been granted leave of absence to study neurology in a number of European centers.

DR. G. S. BRYAN, of the department of botany, and Professor R. J. Roark, of the College of Engineering, at the University of Wisconsin, have been granted leave from their university duties to explore and hunt in eastern Africa during the spring and summer of 1927. They will sail from New York on February 18 for Tanga on the eastern shore of the Red Sea.

DR. FREDERICK L. HOFFMAN, consulting statistician for the Prudential Insurance Company, has been appointed one of the vice-presidents of the forthcoming congress of the Royal Institute of Public Health, which will be held in Ghent, Belgium, during the first week in June. Dr. Hoffman subsequently will attend the International Actuarial Congress in London, during the last week in June, making in the meantime a visit to Geneva to confer with the Cancer and Malaria Committees of the League of Nations and the International Labor Office. From London he will go to Berlin and subsequently to Moscow and other parts of Russia, returning via Finland and Scandinavia to Hamburg and New York by about August 15. Much of his attention during his stay in Europe will be given to aviation hazards.

DR. DEAN F. SMILEY, assistant professor of hygiene at Cornell University, has received a leave of absence to permit him to cooperate with the Carnegie Foundation for the Advancement of Teaching in its inquiry concerning American school, college and university athletics.

AT the University of Texas, Professor William Morris Davis has completed his service as visiting professor in geology for the fall term. During the winter term Professor G. D. Harris, of Cornell University, will give a course in tertiary geology and paleontology.

PROFESSOR EJNAR HERTZSPRUNG, of Leiden University, Holland, and Professor Boris Gerasimovich, of the University of Kharkow, Ukraine, are doing research work at the college observatory at Harvard University.

DR. JIRI V. DANES, professor of general geography at the faculty of natural sciences, Charles University, Prague, expects to spend the academic year, 1927–28, in the United States and to lecture at American colleges and universities. Arrangements for Dr. Danes's lectures are being made by the Institute of International Education, 2 West 45th Street, New York City.

FRANZ X. SCHAFFER, professor of geology and paleontology in the University of Vienna, will lecture at the six weeks' Los Angeles summer session of the University of California beginning June 27. Since 1913 Professor Schaffer has been director of the geological-paleontological department of the Museum of Natural History of Vienna and has recently occupied the president's chair of the Geological Society of Vienna.

THE University of Michigan has invited Professor Sven D. Wicksell, of Lund University, to lecture there during the coming academic year on mathematical statistics.

DR. GEORGE H. F. NUTTALL, Quick professor of biology and director of the Molteno Institute for Parasitology at Cambridge, England, visited the University of Illinois recently. He delivered the Gehrman lecture at the college of medicine in Chicago on January 11, his subject being "Insects and Disease." He lectured in Urbana on the three following days. His first topic, "The University of Cambridge," was presented in the auditorium to a general university audience. The following lectures on Ticks and on Piroplasms were given before groups of advanced students and faculty members.

DR. HENRÉ FREDERICQ, director of the institute of physiology, University of Liége, Belgium, gave the eighteenth Hanna lecture at the Cleveland Medical Library Association, on January 18, on "Humoral Transmission of Nervous Action," illustrated with lantern slides.

PROFESSOR THEODOR VON KARMAN, dean of the Aerodynamic Institute of the University of Aachen, Germany, gave a series of six lectures at the U. S. National Museum on "Modern Development of Aerodynamic Theories," during December, under the auspices of the Daniel Guggenheim Fund for the Promotion of Aeronautics.

DR. JOSEPH ERLANGER, professor of physiology at Washington University, St. Louis, will deliver the fourth Harvey Society Lecture, at the New York Academy of Medicine, on February 19, at 8:30. His subject will be "The Analysis of the Action Potential in Nerve."

ON February 7, at 8:15, in Schermerborn Hall, Columbia University, Dr. Carl E. Seashore, dean of the graduate school of the State University of Iowa, will lecture on "The Musical Mind," before the Columbia Chapter of the Sigma Xi.

DR. EDWIN G. CONKLIN, professor of biology at Princeton University, addressed the medical history section of the Philadelphia College of Physicians, on January 27, on "Heredity and Environment in Human Progress."

DR. CHARLES G. ABBOT, director of the Astrophysical Observatory of the Smithsonian Institution, gave

a lecture on "The Sun as the Fountain of Life," at the Case School of Applied Science, on January 11.

DR. LOUIS O. KUNKEL, of the Boyce Thompson Institute, Yonkers, N. Y., gave the first Mayo Foundation lecture in a series on plant pathology and physiology in relation to man, in Rochester on the evening of January 14. His subject was "Filtrable Viruses."

ON January 8, Professor W. L. Holman, of the department of bacteriology at the University of Toronto, delivered an address to the Royal Canadian Institute on the subject "The Microbe and Civilization." On January 15, A. G. Mörzer-Bruyns, formerly agricultural expert for the Dutch government, at the Hague, gave an address on "Canada—Holland, Agriculture and Immigration."

DR. FRANCIS E. LLOYD, professor of botany at McGill University and president of the American Society of Plant Physiologists, lectured on Spirogyra before the Science Club of St. Lawrence University on January 20. The lecture was illustrated with his original lantern slides and moving pictures.

ON January 12, A. L. Kimball, Jr., of the research laboratories of the General Electric Company, Schenectady, gave a lecture before the staff and students of the department of physics at Amherst College on the subject of "The Internal Friction of Solids."

DR. J. B. SUMNER, assistant professor of biochemistry in the Cornell Medical School, Ithaca division, gave a talk before the Cornell section of the American Chemical Society on January 19 on "The Isolation of the Enzyme Urease."

DR. E. E. FREE, consulting engineer and lecturer at New York University, gave a lecture on January 28 before the New York Electrical Society on "Scientific Conquests of 1926."

DR. E. P. CHURCHILL, head of the department of zoology in the University of South Dakota, delivered an illustrated lecture on the "Evolution of Fishes" before the Sioux City (Iowa) Academy of Science and Letters on February 1.

THE Chicago Medical Society held a memorial meeting in commemoration of the late Dr. Albert J. Ochsner on February 3. Dr. Walter W. Chipman, Montreal, Canada, spoke on "A Personal Appreciation of Ochsner"; Dr. William J. Mayo, Rochester, Minn., on "Ochsner's Work," and Dr. Allen B. Kanavel, Chicago, on "Surgical Progress." This is the first lecture of the Ochsner Memorial lectures established by the northside branch of the society.

PROFESSOR CHARLES C. NUTTING, head of the department of zoology at the University of Iowa, has died at the age of sixty-eight years.

DR. E. T. DUMBLE, consulting geologist of Texas and formerly state geologist, died on January 27, aged seventy-four years.

PROFESSOR ALBERT H. TUTTLE, from 1888 until his retirement in 1913 head of the school of biology at the University of Virginia, died on January 23, aged eighty-two years.

ARTHUR NEWTON, for more than twenty years associate astronomer in the United States Naval Observatory, has died, aged fifty-nine years.

DR. FRANKLIN DEXTER, formerly associate professor of anatomy in the Harvard Medical School, died on January 18 at the age of sixty-nine years.

SIR WILLIAM AUGUSTUS TILDEN, F.R.S., emeritus professor of chemistry in the Imperial College of Science and Technology, England, died on December 11, in his eighty-fifth year.

DR. LEONCE PIERRE MANOUVRIER, professor of physiological anthropology in the School of Anthropology in Paris, died on January 18, aged seventy-six years.

PROFESSOR YOVAN TZVYITCH, president of the Yugoslav Academy of Science, died on January 16. He was well known for his geographical, geological and ethnographical studies.

THE death is reported from Zurich, at the age of forty-eight years, of Professor Alfred de Quervain, the meteorologist. From 1902 to 1906 he was secretary to the International Commission which investigated the highest meteorological zones of the atmosphere and was the leader of the Swiss expedition to Greenland.

THE United States Civil Service Commission announces an open competitive examination for associate sanitary engineer, applications for which must be on file at Washington, D. C., not later than February 21. The examination is to fill a vacancy in the United States Veterans' Bureau, and vacancies occurring in positions requiring similar qualifications, at an entrance salary of $3,000 a year.

THE success of last year's experiment in holding a Mid-West power show in Chicago has led to the carrying out of plans for a more extensive exhibition of this kind this year. It will be held on February 15, 16, 17 and 18 in the Coliseum in Chicago, whose 82,000 square feet of floor space will permit the convenient arrangement of the large and diversified exhibit of power machinery and allied material. At least 260 companies will participate, showing equipment worth over $1,000,000. The technical program represents the second annual Mid-West Power Conference, which has the cooperation of the leading engineering societies.

DR. JOHN R. NEAL, senior counsel for John T. Scopes, filed a petition on January 25 with L. D. Smith, state attorney general, to protect the interests of the defense pending an agreement among associate counsel as to the next move. The petition requested a rehearing in the anti-evolution case which the state has nolle prossed. It is reported that the Tennessee Supreme Court has declined to consider the petition.

AN anti-evolution bill was introduced in the Missouri legislature on January 18. The proposed law would prevent "the teaching of any theory or hypothesis in regard to the origin of life on this planet in the public schools that is contradictory of the divine account of creation as set forth in the first and second chapters of Genesis in the Holy Bible."

A RESOLUTION which would have prohibited the teaching of evolution in West Virginia's public schools was defeated in the house of delegates on January 21. The vote on the resolution was 57 to 36.

THE extensive collection of Coleoptera accumulated by the late Mr. Fred C. Bowditch, of Brookline, Mass., has been presented to the Museum of Comparative Zoology by his family.

UNIVERSITY AND EDUCATIONAL NOTES

GROUND has been broken for the new laboratories of chemistry at Harvard University which will consist of two buildings connected by a tunnel. The first and larger of these is to go up on Oxford Street between the new lecture hall and the university museum, while the second will be immediately behind it on Frisbie Place.

THE estate of Dr. Clarence A. McWilliams will go to Princeton University after the death of his two sisters, who share the $75,000 estate during their lives.

BY the will of the late Colonel George R. Hooper, of Montreal, McGill University will receive $100,000.

WASHINGTON COLLEGE, Maryland, has received a gift of $30,000 from a friend of the college, whose name has been withheld, to endow a chair of mathematics in honor of J. S. William Jones, dean of the faculty.

DR. S. CHARLETY, rector of the University of Strasbourg, has been named rector of the Sorbonne to succeed Dr. Paul Lapie, who died on January 24.

DR. WILBURT CORNELL DAVISON, associate professor of pediatrics and assistant dean of the medical school at the Johns Hopkins University, has been appointed dean of the new school of medicine to be established at Duke University. Dr. Davison will at once assume responsibility of the organization of the school and the construction of the hospital and medical college buildings.

PROFESSOR H. S. TAYLOR has been appointed chairman of the department of chemistry of Princeton University upon the resignation of Professor Lauder W. Jones. Professor Jones continues as director of research and teaching of organic chemistry in the university.

DR. WILLIAM MCDOUGALL, professor of psychology at Harvard University, has resigned to accept a position on the faculty of Duke University, Durham, N. C.

DR. CYRUS C. STURGIS, assistant professor of medicine at the Harvard Medical School and an associate at the Peter Bent Brigham Hospital, has been appointed professor of internal medicine and director of the new Simpson Memorial Institute at the University of Michigan.

ASSOCIATE PROFESSOR ABRAHAM COHEN, of the Johns Hopkins University, has been promoted to a full professorship of mathematics.

DR. WILLIAM SOMERVILLE, Sibthorpian professor of rural economy at Oxford University, has retired and has been succeeded by J. A. S. Watson, professor of agriculture and rural economy of the University of Edinburgh.

M. EUGENE BLOCH has succeded M. Leduc as professor of theoretical physics at the University of Paris.

DISCUSSION AND CORRESPONDENCE

"THE QUANTITATIVE THEORY OF SEX"

UNDER the above title R. Goldschmidt in a recent issue of SCIENCE[1] writes that "*The quantitative theory of sex* was first derived by the present writer in 1912 (preliminary note in 1911), in essentially the same form as it stands to-day, from his experiments on intersexuality in the gipsy moth. (The term intersexuality was only used since 1915.) The theory claims that in both sexes determiners for femaleness and maleness are present, the relative quantities of which are balanced in such a way that one or the other has the upper hand in the respective sexes. Which of them is to be present in the higher quantity is decided by the mechanism of the sex chromosomes, etc." He further states that "during the years between 1912 and 1922 the Columbia group of Drosophila workers was solidly opposed" to his theory of sex; that this group of workers has since arrived at conclusions essentially confirmatory of his theory and that they should have made or should now make such

[1] SCIENCE, n. s., Vol. 64, p. 299, 1926.

acknowledgement. Goldschmidt makes no reference to the work of the present writer.

If Goldschmidt had written under another title, namely, "A Quantitative Theory of *Normal* Sex-determination," his discussion would afford less cause for my comment. On this last-mentioned subject—but not on "the quantitative theory of sex"—Goldschmidt can properly claim precedence, since he found genes influencing sexuality in the autosomes and obtained evidence that these determiners were not of equal potency in various races or species of moths. But those contributions are of very subsidiary importance—indeed they are unimportant—to "the quantitative theory of sex," as this has developed during the last fifteen years in quite other hands than those of Goldschmidt. This particular theory rests essentially on the demonstrated fact that the entire normal genetic equipment (or the chromosomal determiners) for femaleness may, under experiment, be made to produce a *male,* and *vice versa;* and that intermediate stages of sexuality may be thus produced. In this basic fact it is relatively unimportant whether in addition to determiners for sex in the X- and Y-chromosomes there are other determiners in the autosomes; and equally immaterial whether these factors or determiners are of constant or of variable potency in various races or species. In either case the fact of first importance is that the complete and *normal* factorial basis of the one sex has been forced to deliver the opposite sex. Whatever the facts of normal sex-determination, they form but a part—here an unimportant one—of the whole of the present question of *the quantitative nature of sexuality.*

The first evidence definitely interpreted as involving the essential fact of sex-reversal (along with a quantitative basis of sex as well) was presented by the writer in 1912[2] (paper before the American Society of Zoologists, 1911). Other evidence was given in 1914,[3,4] 1916,[5] 1917[6] and in several later papers. In succeeding years true sex-reversal has been accomplished, and so interpreted, in several animal groups by several investigators. It is now a well-established fact.

The second point of importance to "the quantitative theory of sex" (as many or most students of sexuality now regard it) is the following: What is the essential thing about sex that is quantitated? That this is *not* the complete normal genetic equipment (determiners) for normal sex-determination is plain, since as just noted above, this equipment for one sex can be made to yield the opposite or an intermediate sex. Probably this *is* the same thing that overrides the genetic equipment (also upon which the determiners normally act) and enforces the development of the opposite sex. The first evidence concerning the functional unit or process that overrides the genetic basis of sex was presented by the writer[2] in 1912 (to the American Society of Zoologists, 1911). Other evidence was given in 1914,[4] 1916,[5] 1917[6,7] and in later papers. This functional unit was found by me to be *metabolic rate.*

In 1912 (p. 463) we could give a bit of evidence that the male-producing ova of the pigeon were distinguished from female-producing ova "by smaller size, higher water content, and smaller energy content (later written as less *stored* energy)." In 1914[4] (p. 276) this size difference was confirmed and it was specifically stated that "a high *storage* power in the egg is, however, probably an index of lower *oxidizing* capacity." In later papers these points have been rather fully developed, and those American readers of SCIENCE who are students of the sex problem know that the writer has been continuously engaged in a study of this particular aspect of the problem for nearly sixteen years. Several investigators have accepted part or much of this quantitative and metabolic theory of sex.

Indeed, it may next be noted that Goldschmidt himself has somewhat belatedly accepted important parts of our theory, though he has nowhere acknowledged it. In 1916[8] he made his *first* attempt to identify his determiners (of various potencies in various races) with a functional unit or process—and chose to call them "enzymes." In 1917[9] he again considered the "male and female enzymes" as *qualitatively* different and chose to call these hypothetical bodies andrase and gynase. He had, of course, neither found nor measured variable quantities of "enzymes" associated with sex determiners of variable potency. It was wholly speculative. Our special point here, however, is that if this speculation were true it would become a beautiful evidence for the wide applicability of the metabolic theory of sex, as sponsored by myself. Enzymes, many hormones and all catalysts of whatever kind—even inorganic ones—are but tools and handmaidens in the garden of metabolism; though several writers on sex phenomena have seemed en-

[2] Riddle, O., SCIENCE, n. s., Vol. 35, p. 462, 1912. (Abstract.)

[3] Riddle, O., SCIENCE, n. s., Vol. 39, p. 440, 1914.

[4] Riddle, O., *Amer. Acad. Med.*, Vol. 15, p. 265, 1914.

[5] Riddle, O., *Amer. Nat.*, Vol. 50, p. 385, 1916.

[6] Riddle, O., *Jour.* Wash. Acad. Sci., Vol. 12, p. 319, 1917.

[7] Riddle, O., SCIENCE, n. s., Vol. 46, p. 19, 1917.

[8] Goldschmidt, R., SCIENCE, n. s., Vol. 43, p. 98, 1916.

[9] Goldschmidt, R., *Jour. Exp. Zool.*, Vol. 22, p. 593, 1917.

tirely unconscious of this fact. Later, in 1919, "hormones" (again without identifications or measurements) are added to Goldschmidt's theory of sex-development as extended to vertebrates. And, finally, by 1923, in this latter field even the word *metabolism* (no measurements) is sometimes agreeably utilized. Such definite progress is of course gratifying to the writer.

From the preceding it may appear that the writer has contributed something to the "quantitative theory of sex." Personally, I am fairly indifferent toward "acknowledgments" in favor of myself. But if it is desirable for the "Columbia group of Drosophila workers who solidly opposed" Goldschmidt's theory of sex-determination now "to make a frank statement" of its acceptance, I can merely suggest that the little matter of acknowledgments might well be extended beyond the Columbia group.

It remains to note that the past fifteen years have provided so much advance in the problem of sexuality, from contributions of so many distinct kinds, that it is probably too much to expect any single individual to be aware of the total advance. At any rate no one gives evidence of knowing more than a fraction of the available data. Perhaps the interests of the subject and of individual investigators of it will be adequately served if the main lines of progress are known, and if a part is not too often mistaken for the whole.

OSCAR RIDDLE
STATION FOR EXPERIMENTAL EVOLUTION,
COLD SPRING HARBOR, L. I.

GOLDSCHMIDT'S article on "The Quantitative Theory of Sex" in SCIENCE for September 24 recalled to the writer strongly, though indirectly, phenomena of sex that deserve careful and general attention.

Extended observations on human material, body, crania, bones, organs, appendages, show that sex in man manifests itself not as a uniform but as a variable character. And its variability compares with that due to other fundamental factors, such as age, race, etc., and with that of individual organic features in general.

The female as well as the male sex in man, under even the most normal conditions, is a broad stream of variations, so that one woman is by no means sexually equal to another woman, or one man to another man. The ranges of sexual variation in individual features, as well as that of sexuality as a whole, give, if we have sufficient numbers, regular binomial curves of distribution, with their usual characteristics; and these curves in their extremes do not merely approach but overlap each other. Thus there are females of hyper- to hypo-feminine, with males of hypo- to hyper-masculine grades, and the least feminine individuals stand no more apart but interdigitate with or overlap, in many respects, the least masculine, so as to be separable perhaps only by the main sex organs; while the most masculine and feminine are decidedly farther apart than the large majority, who may be termed more or less orthosexual.

Perhaps the simplest demonstration of these facts, which must be of much genetic as they are of other significance, is obtained on the human skull and bones. Taking the individual larger bones of the skeleton, the pelvis, the lower jaw, or the skull, an experienced worker will be able to positively identify as to sex from approximately 50 to 93 per cent. of the specimens, according to their kind. The pelvis, and the skull with its lower jaw, give the highest proportion of identifiable cases, yet even of these on the average about seven in each hundred present features either so intermediate that the sex remains uncertain, or approaching so close to those of the other sex that an erroneous identification is possible. Even where the complete skeleton is present there are still two to three cases in every hundred in which it is impossible to be certain whether the remains are those of a male or a female. On the other hand there are specimens of which the sex is most patent, is in fact exaggerated; while a large majority range between these two extremes. And what is true of bones is true of brains and probably of all other organs, except the genital; though even in the latter there is ample variation.

The subject of sex, seen in this light, rouses much thought; but the object of this note is merely to call attention to the broadness of the unit character of sex; to the merging of the male and female characteristics on the confines of their normal variation; and to the similarity of their behavior with that of all other organic manifestations in man, as well as, doubtless, in other living forms. Perhaps this may call for a re-atunement of our views on the sex question.

ALEŠ HRDLIČKA
U. S. NATIONAL MUSEUM

SOME NOTES ON THE "ELDEN PUEBLO"

IN a recent number of SCIENCE (November 19, 1926) among the proceedings of the National Academy of Sciences meeting at Philadelphia, Dr. J. Walter Fewkes has presented a paper on the "Elden Pueblo" which contains several confusing statements. These have to do with its discovery and selection of the name. The author writes of the ruin: "The object of the communication was to announce the discovery by archeological methods, of a pre-historic ruin which had never been described. . . . It was

acknowledgement. Goldschmidt makes no reference to the work of the present writer.

If Goldschmidt had written under another title, namely, "A Quantitative Theory of *Normal* Sex-determination," his discussion would afford less cause for my comment. On this last-mentioned subject—but not on "the quantitative theory of sex"—Goldschmidt can properly claim precedence, since he found genes influencing sexuality in the autosomes and obtained evidence that these determiners were not of equal potency in various races or species of moths. But those contributions are of very subsidiary importance—indeed they are unimportant—to "the quantitative theory of sex," as this has developed during the last fifteen years in quite other hands than those of Goldschmidt. This particular theory rests essentially on the demonstrated fact that the entire normal genetic equipment (or the chromosomal determiners) for femaleness may, under experiment, be made to produce a *male,* and *vice versa;* and that intermediate stages of sexuality may be thus produced. In this basic fact it is relatively unimportant whether in addition to determiners for sex in the X- and Y-chromosomes there are other determiners in the autosomes; and equally immaterial whether these factors or determiners are of constant or of variable potency in various races or species. In either case the fact of first importance is that the complete and *normal* factorial basis of the one sex has been forced to deliver the opposite sex. Whatever the facts of normal sex-determination, they form but a part—here an unimportant one—of the whole of the present question of *the quantitative nature of sexuality.*

The first evidence definitely interpreted as involving the essential fact of sex-reversal (along with a quantitative basis of sex as well) was presented by the writer in 1912[2] (paper before the American Society of Zoologists, 1911). Other evidence was given in 1914,[3,4] 1916,[5] 1917[6] and in several later papers. In succeeding years true sex-reversal has been accomplished, and so interpreted, in several animal groups by several investigators. It is now a well-established fact.

The second point of importance to "the quantitative theory of sex" (as many or most students of sexuality now regard it) is the following: What is the essential thing about sex that is quantitated? That this is *not* the complete normal genetic equipment (determiners) for normal sex-determination is plain, since as just noted above, this equipment for one sex can be made to yield the opposite or an intermediate sex. Probably this *is* the same thing that overrides the genetic equipment (also upon which the determiners normally act) and enforces the development of the opposite sex. The first evidence concerning the functional unit or process that overrides the genetic basis of sex was presented by the writer[2] in 1912 (to the American Society of Zoologists, 1911). Other evidence was given in 1914,[4] 1916,[5] 1917[6,7] and in later papers. This functional unit was found by me to be *metabolic rate.*

In 1912 (p. 463) we could give a bit of evidence that the male-producing ova of the pigeon were distinguished from female-producing ova "by smaller size, higher water content, and smaller energy content (later written as less *stored* energy)." In 1914[4] (p. 276) this size difference was confirmed and it was specifically stated that "a high *storage* power in the egg is, however, probably an index of lower *oxidizing* capacity." In later papers these points have been rather fully developed, and those American readers of SCIENCE who are students of the sex problem know that the writer has been continuously engaged in a study of this particular aspect of the problem for nearly sixteen years. Several investigators have accepted part or much of this quantitative and metabolic theory of sex.

Indeed, it may next be noted that Goldschmidt himself has somewhat belatedly accepted important parts of our theory, though he has nowhere acknowledged it. In 1916[8] he made his *first* attempt to identify his determiners (of various potencies in various races) with a functional unit or process—and chose to call them "enzymes." In 1917[9] he again considered the "male and female enzymes" as *qualitatively* different and chose to call these hypothetical bodies andrase and gynase. He had, of course, neither found nor measured variable quantities of "enzymes" associated with sex determiners of variable potency. It was wholly speculative. Our special point here, however, is that if this speculation were true it would become a beautiful evidence for the wide applicability of the metabolic theory of sex, as sponsored by myself. Enzymes, many hormones and all catalysts of whatever kind—even inorganic ones—are but tools and handmaidens in the garden of metabolism; though several writers on sex phenomena have seemed en-

[2] Riddle, O., SCIENCE, n. s., Vol. 35, p. 462, 1912. (Abstract.)

[3] Riddle, O., SCIENCE, n. s., Vol. 39, p. 440, 1914.

[4] Riddle, O., *Amer. Acad. Med.,* Vol. 15, p. 265, 1914.

[5] Riddle, O., *Amer. Nat.,* Vol. 50, p. 385, 1916.

[6] Riddle, O., *Jour.* Wash. Acad. Sci., Vol. 12, p. 319, 1917.

[7] Riddle, O., SCIENCE, n. s., Vol. 46, p. 19, 1917.

[8] Goldschmidt, R., SCIENCE, n. s., Vol. 43, p. 98, 1916.

[9] Goldschmidt, R., *Jour. Exp. Zool.,* Vol. 22, p. 593, 1917.

tirely unconscious of this fact. Later, in 1919, "hormones" (again without identifications or measurements) are added to Goldschmidt's theory of sex-development as extended to vertebrates. And, finally, by 1923, in this latter field even the word *metabolism* (no measurements) is sometimes agreeably utilized. Such definite progress is of course gratifying to the writer.

From the preceding it may appear that the writer has contributed something to the "quantitative theory of sex." Personally, I am fairly indifferent toward "acknowledgments" in favor of myself. But if it is desirable for the "Columbia group of Drosophila workers who solidly opposed" Goldschmidt's theory of sex-determination now "to make a frank statement" of its acceptance, I can merely suggest that the little matter of acknowledgments might well be extended beyond the Columbia group.

It remains to note that the past fifteen years have provided so much advance in the problem of sexuality, from contributions of so many distinct kinds, that it is probably too much to expect any single individual to be aware of the total advance. At any rate no one gives evidence of knowing more than a fraction of the available data. Perhaps the interests of the subject and of individual investigators of it will be adequately served if the main lines of progress are known, and if a part is not too often mistaken for the whole.

OSCAR RIDDLE
STATION FOR EXPERIMENTAL EVOLUTION,
COLD SPRING HARBOR, L. I.

GOLDSCHMIDT'S article on "The Quantitative Theory of Sex" in SCIENCE for September 24 recalled to the writer strongly, though indirectly, phenomena of sex that deserve careful and general attention.

Extended observations on human material, body, crania, bones, organs, appendages, show that sex in man manifests itself not as a uniform but as a variable character. And its variability compares with that due to other fundamental factors, such as age, race, etc., and with that of individual organic features in general.

The female as well as the male sex in man, under even the most normal conditions, is a broad stream of variations, so that one woman is by no means sexually equal to another woman, or one man to another man. The ranges of sexual variation in individual features, as well as that of sexuality as a whole, give, if we have sufficient numbers, regular binomial curves of distribution, with their usual characteristics; and these curves in their extremes do not merely approach but overlap each other. Thus there are females of hyper- to hypo-feminine, with males of hypo- to hyper-masculine grades, and the least feminine individuals stand no more apart but interdigitate with or overlap, in many respects, the least masculine, so as to be separable perhaps only by the main sex organs; while the most masculine and feminine are decidedly farther apart than the large majority, who may be termed more or less orthosexual.

Perhaps the simplest demonstration of these facts, which must be of much genetic as they are of other significance, is obtained on the human skull and bones. Taking the individual larger bones of the skeleton, the pelvis, the lower jaw, or the skull, an experienced worker will be able to positively identify as to sex from approximately 50 to 93 per cent. of the specimens, according to their kind. The pelvis, and the skull with its lower jaw, give the highest proportion of identifiable cases, yet even of these on the average about seven in each hundred present features either so intermediate that the sex remains uncertain, or approaching so close to those of the other sex that an erroneous identification is possible. Even where the complete skeleton is present there are still two to three cases in every hundred in which it is impossible to be certain whether the remains are those of a male or a female. On the other hand there are specimens of which the sex is most patent, is in fact exaggerated; while a large majority range between these two extremes. And what is true of bones is true of brains and probably of all other organs, except the genital; though even in the latter there is ample variation.

The subject of sex, seen in this light, rouses much thought; but the object of this note is merely to call attention to the broadness of the unit character of sex; to the merging of the male and female characteristics on the confines of their normal variation; and to the similarity of their behavior with that of all other organic manifestations in man, as well as, doubtless, in other living forms. Perhaps this may call for a re-atunement of our views on the sex question.

ALEŠ HRDLIČKA
U. S. NATIONAL MUSEUM

SOME NOTES ON THE "ELDEN PUEBLO"

IN a recent number of SCIENCE (November 19, 1926) among the proceedings of the National Academy of Sciences meeting at Philadelphia, Dr. J. Walter Fewkes has presented a paper on the "Elden Pueblo" which contains several confusing statements. These have to do with its discovery and selection of the name. The author writes of the ruin: "The object of the communication was to announce the discovery by archeological methods, of a pre-historic ruin which had never been described. . . . It was

practically unknown to any scientific man before May, although the artificial appearance of the clearing in the pines had been recognized as the site of a settlement from the time that Flagstaff was settled by white people."

If the author would refer to the Memoirs of the American Anthropological Association, (1918) volume V, number 4, he would find on page 115 the ruin described as pueblo 142 and figured on plate X, figure 1. In a manuscript of an intensive survey of the ruins of the San Francisco Mountain region which has been in the hands of the Bureau of Ethnology for the past year and a half awaiting publication, this ruin is again described, furnished with a measured plan and given a name. Indeed, Dr. Fewkes undertook the excavation of the ruin at the suggestion of the writer and used the measured plan referred to above in the early part of his excavations.

Dr. Fewkes says further, "The name 'Elden Pueblo' was given to the ruin by the author on account of its neighborhood to Elden Mesa. . . ." The name "Elden Pueblo" is ill-advised because in the manuscript of the survey referred to above, which is in the hands of Dr. Fewkes, a pueblo ruin west of Elden spring is called the *Elden Pueblo* and appropriately so because this pueblo is on the very flanks of Elden Mountain, while Fewkes's "Elden Pueblo" lies nearly a mile away, and a quarter of a mile from the conspicuous crater of Sheep Hill. Therefore, the writer of the survey has called Fewkes's ruin the *Sheep Hill Pueblo*. In the Memoirs of the American Anthropological Association, referred to above, a third site is called the *Elden Spring Pueblo*. Surely it is confusing to the literature to give another pueblo the name of Elden.

HAROLD S. COLTON
UNIVERSITY OF PENNSYLVANIA

PRESBYOPIC VISION AS AN INDEX OF LONGEVITY

THE article on "Age of Presbyopic Vision as an Index of the Longevity of Primitive Man," in SCIENCE of October 29, seems to me to have overlooked one vital point. Any character to be of survival value, either positive or negative, must appear before the close of the reproductive period. Presbyopia appears after the reproductive period is practically past, hence can not affect the next generation. What occurs to parents after their offspring are independent of them has no effect on the survival of the race. This is illustrated by many animals that die in producing their young, as is the case with the salmon.

It would seem more logical to connect the age at which presbyopia appears with the end of the reproductive period. Any family in which it appeared before the offspring were independent would be at a disadvantage and would tend to be eliminated. Thus natural selection has prevented presbyopia appearing before the middle forties. Whatever theory may be correct, it is a fact that these two events occur at practically the same time.

C. J. ELMORE
WILLIAM JEWELL COLLEGE,
LIBERTY, MISSOURI

SIMPLIFIED SPELLING

WHY not take the obvious additional step to that proposed by Maynard M. Metcalf for "A Simplified Indication for the Consonant Sound Represented by the Letters TH" (SCIENCE, 1670, page 650) and drop the *u* that invariably follows *q* in English!

A. H. BABCOCK
SAN FRANCISCO, CALIF.

SCIENTIFIC BOOKS

Astronomy. By RUSSELL, DUGAN and STEWART. Vol. 1—The Solar System. (470 + xxi.) Ginn and Co.

THIS work is sub-titled "A Revision of Young's Manual of Astronomy." Young's text-books on astronomy were about as near perfection as they could be at the time they were written. His "Manual" is extremely well adapted for a first, general college course, and his "General Astronomy" is as admirable for the student starting on the more serious study of the subject. All that was needed for the present time was a revision of his works to bring them up to date, to give an account of the more recent advances in the science. This the authors, the successors of Professor Young at Princeton University, have undertaken.

The revision of the "Manual" has resulted in a more extensive work than the original, so that we have before us a text, as the authors state, "intermediate between this and the 'General Astronomy.'" They further state that "extensive changes have been required by the progress of the science; the book has been practically rewritten and inevitably increased in length." On account of the increase in length the work is now issued in two volumes. I have been informed that the second volume is in press. The first has just appeared.

The division of the work into two volumes is advantageous for two reasons: first, in that the subject-matter of the first volume is of the more stable sort and will not need revision for many years; and the subject-matter that is in the most rapid state of flux is to be in the second volume, so that the work can be kept up to date by fairly frequent revisions of the second volume only; second, the division fits admir-

bly with the way the subject is presented here in the niversity of California. Our first course is prac-ically on the solar system, which is covered by the rst volume, and our second course is on modern stronomy, which is undoubtedly covered by the sec-nd volume soon to be issued.

This first volume is extremely well done. The only dverse criticism I have to make is that it is not quite omplete. Undoubtedly the second volume furnishes he material some of which I would like to see in the rst volume. Even at the expense of a slight repeti-ion of this material to be placed in the second vol-me, I would have had a chapter treating of the solar ystem in the sidereal universe. Such a subject, for nstance, as "The Sun's Way" should not be omitted rom a volume on the solar system. Space for such n additional chapter might well be gained by short-ning the chapter on "Celestial Mechanics." Much f the subject-matter of this chapter is quite beyond he grasp of those for whom this work is intended nd finds its more appropriate place in the "General stronomy."

In connection with this chapter on "Celestial echanics" I can not refrain from mentioning the mile with which I read on page 275, "The calcula-ion of an orbit from three observations takes a killed computer two days or sometimes less. The ovice may take as many weeks, most of his time eing occupied in finding and correcting mistakes hich are only too easy to make." With the last sen-ence I heartily agree, but would add that often these eeks of struggle are due to erroneous observations hich, alas, are entirely too frequent. The first sen-ence of this quotation I would have written, "The alculation of an orbit from three good observations akes a skilled computer *a* few hours."

The book is well balanced and very up-to-date, as s evidenced by the introduction of such items as the endulum observations in a submarine.

I am sure that the second volume, which will con-ain many subjects upon which Professor Russell is n eminent authority, will be of the same degree of xcellence as this. The whole work will be not only superb text-book, but also a regular authoritative andbook for the professional astronomer.

The authors have wisely followed Professor Young n giving numerous examples for the student to solve. A most welcome and valuable addition is the frequent ists of references.

I have noted a few very minor corrections which ill be sent directly to the authors.

The book is well illustrated and beautifully printed.

R. T. CRAWFORD

STUDENTS' OBSERVATORY,
UNIVERSITY OF CALIFORNIA

History of the Origin and Establishment of the Inquisition in Portugal. By ALEXANDRE HERCULANO. Translated by JOHN CASPER BRANNER. Stanford University Press.

DR. JOHN CASPER BRANNER, professor of geology in Stanford University for thirty years, and for three years its president, was for nearly half a century in one way or another connected with the Geological Survey of Brazil. In this capacity he acquired a very thorough knowledge of the Portuguese language, in which he published several important works.

After retiring in 1916 from active work in the university, Dr. Branner devoted himself mainly to writing, and among other things he made a translation of one of the most important of Portuguese historical works, Herculano's "History of the Inquisition."

Dr. Herculano gives a just and sane account of the most hideous period in the history of his native country, supported throughout by documents, and forming as a whole the strongest possible arraignment of the form of intolerance called religious, and of the beastly efforts to destroy heresy, current in the sixteenth century. It is much to the credit of modern Portugal that its scholars are free to tell the truth, which "Absolution" and "Patriotism" would like to conceal.

DAVID STARR JORDAN

SCIENTIFIC APPARATUS AND LABORATORY METHODS

A MICRO METHOD FOR ESTIMATING THE RELATIVE DISTRIBUTION OF GLUTATHIONE IN INSECTS

HOPKINS[1] in 1921 isolated a constituent of cell protoplasm responsible for the nitroprusside reaction in animal tissues; this nitroprusside test had been previously applied by Heffter[2] and Arnold[3] in proof of the presence of a sulphydryl group (SH) in the cell. According to Hopkins, the substance which he isolated and named "glutathione" is a dipeptide and contains glutamic acid and cystein. This dipeptide is autoxidisable; it acts readily under varying conditions either as a hydrogen or oxygen acceptor, promoting cell oxidation and reduction under factors present in the tissues, and presumably has actual functions in the chemical dynamics of the cell.

During the writer's studies with arsenicals, the effect of the latter upon respiratory metabolism of insects indicated that in general the oxygen consumption and carbon dioxide production were profoundly

[1] Hopkins, *Biochem. Jour.*, 15, 286, 1921.

[2] Heffter, Med. Naturwiss Arch. 1, 81, Maly's Jahresb. 1908.

[3] Arnold, *Zeitsch. Physiol. Chem.*, 70, 300 and 314, 1911.

inhibited. It seemed therefore of considerable importance to determine the substance in the cells of tissues and organs singularly affected when insects are subjected to arsenical treatment. If glutathione has such tremendous function in cell activity as formulated by the above-mentioned workers and others, it was thought profitable to investigate its presence, distribution and reaction towards arsenic in insects.

For quantitative determinations of the presence of glutathione and the effect that arsenicals have upon the glutathione content of insects, the method used was the one described by Tunnicliffe[4] on the quantitative determination of glutathione in yeast. This method, however, necessitates the use of large quantities of material, and the results of these investigations will be described in a future paper.

Along with the above studies it appeared important to obtain a knowledge of the relative distribution of glutathione in the tissues and organs of an insect, and it was therefore found desirable to adopt the suggestions incorporated in the work of Heffter and Hopkins. As a result, a micro method for the qualitative determination of glutathione in insects was adopted by the writer in his investigations, as follows:

In a petri dish or similar receptacle a layer of paraffin about a centimeter thick is poured and left to harden; a little of the paraffin from the center of the dish is scooped out to form a depression for the reception of about 5 cc of a saturated solution of ammonium sulphate $(NH_4)_2SO_4$. The petri dish is then mounted on the stage of a binocular dissecting microscope and the insect to be tested is dissected in this fluid and the organs are carefully separated. This is best accomplished by removing them entirely from the body cavity, thus exposing the body muscles. The leg muscles may also be exposed by slitting the chitin. If the ammonium sulphate solution becomes discolored during the process of dissection, it can be siphoned off with a pipette and fresh solution added. After slitting the insect, it is very often best to heat it in a little dilute acetic acid for a brief period before placing in the ammonium sulphate solution. About four to six drops of a 5 per cent. solution of sodium nitroprusside $Na_2Fe(CN)_5$ $(NO)2H_2O$, is added to the ammonium sulphate solution, and afterwards about 1 cc of ammonium hydroxide. Upon the addition of the latter a very deep magenta color flashes up in the muscles and various parts of the organs, the brilliancy of the color lasting for several minutes. The observer, by noting the nitroprusside reaction in the various parts of the tissues and organs, obtains a fair knowledge of the distribution of glutathione in the insect body.

The writer made many observations on various species of insects with the following results. Normal insects subjected to the above procedure gave marked nitroprusside reactions in the muscles of the thorax, head, legs and alimentary tract. The coloration was especially brilliant in the stomach and in the sex organs during development, the brilliancy in the latter possibly being due to the active metabolism of the cells. Other observations were made upon insects which had been subjected to arsenical feeding or injected with arsenical dilutions, and the resultant nitroprusside reaction was compared to the intensity of the color reaction in insects under normal conditions. Such qualitative determinations have shown that 0.1 normal As_2O_3 when fed to insects through the mouth inhibits the nitroprusside reaction first in the stomach, then in the muscles of the thorax, head and sex organs. If a large number of insects are treated with arsenicals and observations are made at fifteen minute intervals, there is obtained a picture of a gradual lessening in the intensity of the nitroprusside reaction. As would be expected from such observations, insects that had been longer under the influence of arsenic showed but a faint nitroprusside reaction in organs and tissues normally giving a brilliant reaction.

A few typical examples may be cited from the large number of observations made.

Normal potato beetles (*Leptinotarsa decemlineata* L.) of both sexes dissected and tested for glutathione, as described above, gave brilliant nitroprusside reactions of the stomach, testes, ovarian follicles and the muscles of the thorax and head. The esophagus and hind intestine gave no reaction to nitroprusside. Potato beetles which were fed or injected with 0.1 normal As_2O_3, and examined five hours later, gave an exceedingly faint nitroprusside reaction of the stomach and muscles of the thorax and head, and no reaction of the ovarian tubules, testes or hind intestines.

Similar tests were made with *Popillia japonica* Newm. In this species the entire normal alimentary tract is usually filled with a dark brownish substance and it is therefore necessary to tease out the contents before the nitroprusside test is applied to the digestive tract. In other respects the reactions were similar to those described for the potato beetle.

In normal squash bugs (*Anasa tristis* DeG.) the entire digestive tract gives a nitroprusside reaction, the coloration being brilliant in the stomach and muscles of the thorax and head. Since in this species the sex organs are naturally orange color, it is not possible to observe the nitroprusside reaction.

In the red-legged grasshopper (*Melanoplus femur-rubrum* DeG.) the entire alimentary tract, except the crop, gave a nitroprusside test, the crop being naturally of a deep brownish color. The coloration is brilliant in the stomach, caeca and the muscles of the

[4] Tunnicliffe, *Biochem. Jour.*, 19, 194, 1925.

thorax, head and hind legs, ovarian follicles and testes, and faint in the upper part of the ileum. Grasshoppers which were fed or injected with 0.1 normal As_2O_3 and examined five hours later gave a very faint nitroprusside test in the alimentary tract, body and head muscles, and in the ovarian follicles and testes, but the hind leg muscles gave a brilliant nitroprusside test.

DAVID E. FINK

BUREAU OF ENTOMOLOGY,
U. S. DEPARTMENT OF AGRICULTURE

SPECIAL ARTICLES

THE OILFISH,[1] RUVETTUS PRETIOSUS, AT BERMUDA

WHEN Mowbray (then of the New York Aquarium)[2] returned on January 12, 1925, from a holiday spent in Bermuda he brought with him a four-foot specimen of this rare deep-sea fish and presented it to the American Museum. This specimen had been taken on December 19, 1924, and being recognized as an unusual fish had been purchased by a friend of his and put in cold storage to await his coming. Although it had been in a refrigeration chamber for a month, the fish was in excellent condition when it reached the museum and it was at once photographed, skinned, a cast and later a mannikin made, and still later the prepared skin mounted.

This very interesting fish was first discovered in and described from that ichthyological treasure ground, the Straits of Messina, by Anastasio Cocco in 1829. Eight years later Cantraine took another specimen from the same waters, and being apparently ignorant of Cocco's previous determination, gave what is possibly the best description of the fish ever made, and the first if not the best figure ever drawn. The fish though rare has been repeatedly taken on the northwest shores of the Mediterranean and off the northwest coast of Africa, but records in our waters are few and far between. From this it will be understood that the capture of this relatively unknown fish is a matter of considerable interest ichthyologically. We have in manuscript a fuller paper dealing with the natural history, iconography, distribution and classification of the fish, but as there is no prospect of early publication of this paper, it seems best without further delay to make this faunal record of its capture.

This specimen of *Ruvettus pretiosus* was a spent male measuring four feet long between perpendiculars and weighing 24.5 pounds. It was taken on the night of December 19, 1924, in seventy-five fathoms, six miles east of Bermuda. Mowbray has obtained records of ten other specimens of *Ruvettus* taken at Bermuda, and it seems that if systematic fishing were carried on there for it, at least a fair number of specimens might be obtained. The data concerning these Bermudan specimens will now be set forth in categorical fashion.

The first Bermudan *Ruvettus* of which we have been able to get any data was taken on December 4, 1909. This fish was caught by Watson Lightbourn, while fishing for red snappers in sixty fathoms of water, on the Challenger Bank, eleven miles southwest of Bermuda. It was a male, six feet six inches long and weighed about fifty pounds. In December, 1911, four other specimens were caught by fishermen on the outer banks. Two were taken by Lightbourn on Argus Bank about twenty-five miles south-southwest of Bermuda in 125 fathoms. The largest was six feet eight inches long and weighed about seventy-five pounds. The other two were taken by Peter Anderson at the same time and place but in about seventy-five fathoms. One of these was a small fish, weighing only about twenty pounds—the smallest ever seen at Bermuda, and one of the smallest on record anywhere. Again on March 12, 1912, Lightbourn[3] took on Challenger Bank, another specimen, a female recently spent, weighing about fifty pounds.

No other captures are recorded for a dozen years, until December 19, 1924, when Stanley Pitcher on the pilot boat "Guard," six miles east of Bermuda, noticed large balls of phosphorescent light deep in the black waters at night. Catching up a fishing outfit he baited the hook with a piece of salt codfish and lowered down to about seventy-five fathoms. In a few minutes he hooked a fish which took him nearly an hour to land, and which, when it came to the surface, appeared to be surrounded by a large ball of blue fire. This fish, which was absolutely unknown to the fishermen of the East End, was the one kept in cold storage for Mowbray, and is the one whose skin now forms one of the treasures of the American Museum.

[1] The significance of the name "oilfish" has already been shown in a previous paper by Gudger, "A New Purgative, the Oil of the 'Castor Oil Fish,' Ruvettus." *Boston Medical and Surgical Journal*, 1925, Vol. 192, pp. 107–111. fig.

[2] Since this note was written, Mowbray has gone to Bermuda to take charge of the aquarium there. One of the various problems which he has slated for investigation is the natural history of *Ruvettus* and particularly the matter of its luminosity, to which reference will be made later.

[3] We regret to record the fact that this able fisherman, who has given Mowbray so much natural history data, was recently drowned while fishing.